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ABSTRACT

Because cognition is inherently inaccessible to observers, researchers interested in cognitive approaches to argumentation frequently rely on self-reports for data. Opponents to self-reports argue that such data have very limited value. They claim that when subjects are put in a position where they have to tell more than they can know, they construct plausible answers that are not based on their specific cognitive experiences. An evaluation of the validity of self-reports does reveal certain conditions under which self-reports about cognition are least suspect. First, people do sometimes judge plausibility even when instructed to remember. A second pertinent chunk of evidence shows that people often behave mindlessly, finding that they can follow the appropriate script without paying conscious attention to what they are doing. Third, the effects of consciousness and memory on retrospective verbal reports are crucially affected by the passage of time. Therefore, questions should be asked as soon as possible. The worth of self-reports, therefore, depends almost entirely upon the procedures used to elicit them. Argumentation scholars should be wary of certain potential problems: letting even a few minutes elapse between a thought process and a report of it, seeking data that were never in consciousness, allowing a plausibility search to be a tempting alternative to retrieval efforts, asking for a general description of a process when subjects only experience a specific episode, and allowing evaluation apprehension or demand characteristics to influence responses. (HOD)

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METHODOLOGICAL ISSUES IN STUDYING ARGUMENT-AS-COGNITION

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Abstract

Because cognition is inherently inaccessible to observers, researchers interested in cognitive approaches to argumentation frequently rely on self-reports for data. The recently revitalized assault on such data is reviewed, along with the defenses presented for verbal accounts. Empirical evidence which bears on the issue does not generate any conclusive decision about the validity of self-reports. On grounds of theoretical coherence, the reasonably optimistic position of Ericsson and Simon (1980) is preferred here. Even so, self-reports must be elicited and interpreted with more care than has usually been the case.

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METHODOLOGICAL ISSUES IN STUDYING ARGUMENT-AS-COGNITION

The human mind has a central place in many contemporary communication theories. Though some coherent accounts of communication are able to do without cognition, most theorists feel obliged to specify the psychological roots of human messages and/or the means by which we have private responses to such stimuli (see Fisher, 1978; Littlejohn, 1978; or Dance, 1932, for current surveys). In recent years, the discipline's interest in cognition has begun to seep into the speciality of argumentation. Symbolic interactionism and personal construct theory have spawned several view of argument (e.g., Willard, 1978; Kneupper, 1979); children's mental abilities and development have been related to their argument skills (e.g., Benoit, 1981); a similar orientation has produced findings about the connection between adults' cognitive capacities and their argumentative performance (e.g., O'Keefe & Delia, 1979); a number of experiments have focused directly on reasoning ability (e.g., Jackson, 1982); attempts to model the cognitive processes involved in argument reception have been published (e.g., Hample, 1981); and several appeals for cognition to be taken more seriously in argumentation have also appeared (Brockriede, 1975; Hample, 1980). A chronicle of the various recent cognitive research traditions in this field would be a large undertaking, and will not be attempted here. But plainly, one of the 1970's most obvious legacies to argumentation scholars is an abiding interest in cognition.

Cognition is not simple to observe, however. The actual psychological processes of another person are never directly available to a researcher. Cognition must therefore be reflected or translated into overt behavior in order to become public data. Part of the responsibility for this transfiguration lies with the experimental subject, and part rests with the investigator. Most often, the researcher asks the respondent to provide a verbal self-report on private

events. The disclosures of a client undergoing psychoanalysis, the thinking aloud protocols of undergraduates reading a message, the oral choices made in a perceptual judgment task, and marks on a Likert scale are all examples of verbal self-reports. Researchers view all these things as indices of the subjects' thoughts; respondents, on the other hand, regard them as answers to the researchers' questions. Broadly put, the topic of this paper is whether those two understandings of the subject's verbal behavior are equivalent.

This exploration of the validity of self-reports will proceed in several stages. First, the case against self-reports will be described; the second section of the paper considers the defense. The third part reports a study undertaken in order to prove the validity of self-reports, but which failed to do so. The final division of the essay attempts to make a balanced assessment of self-reports. The reader no doubt realizes that the issues considered here are certainly not restricted to argumentation; the claims and counterclaims will therefore be discussed in the broad terms they deserve. Argumentation will not again be a focus of special concern until the paper's concluding pages.

The Case Against Self-Reports

In an effort to coalesce the various psychological systems to which he objected, Watson termed the work of James, Wundt, Külpe, Titchener and others "introspective psychology" (e.g., Watson, 1925, p. 3; see Boring, 1953). As is well known, Watson's behaviorism took over the prominent place introspectionism had once held, and did so for several reasons (what follows is hardly a full account; see Boring, 1957, for the standard history). Since the objections to introspection turn out to foreshadow the contemporary complaints about self-reports, the disputes from the early years of this century are worth briefly recounting.

Introspection never provided direct, immediate access to consciousness of sensation, its central topic. A smell, sight, taste, sound or touch would be briefly presented, and then a trained subject would describe the sensation. But,

"It could take twenty minutes to describe the conscious content of a second and a half and at the end of that period the observer was cudgeling his brain to recall what had actually happened more than a thousand seconds ago, relying, of course, on inference" (Boring, 1953, p. 174). This dependence on memory was one of several weak links in introspection, and is also a problem with some contemporary self-report methods.

The obvious need for retrospection joined with the respondents' extensive training to produce another vulnerability, one which might now be called demand characteristics and which Titchener (1915, ch. 1) referred to as the handicap of common sense. Boring's (1953) judgment is that the experimenters' instructions inevitably crept into the observers' supposedly objective reports. Ogden (1926, pp. 175-176), too, says that a danger in introspection is that people normally think in terms of hypotheses (his examples are the Self, the Will, Memory, Intelligence, Instincts and the Unconscious) and that none of these are facts. Each of the old labs tried its best to eliminate such sources of bias (for an example of a student manual, see Titchener, 1901), and thus produced the endless and stultifying reports which Boring (1953) describes. Hindsight makes us doubt the essential wisdom of even trying to train adults into a perfect attitude of tabula rasa.

Another fundamental problem is that the experience of a sensation is a different thing than the experience of an experience of a sensation, and the verbal report of the latter is different still. Psychologists wanted data on the first phenomenon, but had to settle, as we do today, for the third (Ogden, 1926, p. 189). The act of introspecting may even make the original event forever unavailable: ". . .what happens when we look inwards is different from what happens when we are not introspecting. Introspection is not simply an uncovering, a revealing of our ordinary experiences; it is a further and a different experience. What seem to be the 'data' of introspection do not occur at all when we are not looking inwards" (Ogden, 1926, p. 192). Certainly we can all perform certain mental tasks, but our accounts of them are impossibly laden with "external" ideas. Furthermore,

we cannot do the mental task at the same time we observe it. Thus Marr (1981, p. 140) avers, "I have no doubt that when we do mental arithmetic we are doing something well, but it is not arithmetic, and we seem far from understanding even one component of what that something is." In this arena, as in others, the act of observing changes the thing observed.

These three difficulties conspired to produce another, more obviously embarrassing one: the various introspective laboratories often failed to produce results which could be replicated in anyone else's lab (Boring, 1953). The strain toward science produced intensive special training for the observers, but these people's minds could not be rinsed clean enough. One cost of this effort after pure sensation was that quite a lot of interesting phenomena were not within the compass of this science: unconscious meaning, goal directed thought, feelings, imageless thought and the forces of Freud's psychodynamics, for instance, all resisted being unfolded by introspection (Marx & Hillix, 1979, pp. 86-87; Boring, 1953). So introspectionism's elaborate training in observation sacrificed scope without securing scientific replicability.

Thus introspection as a theory of psychology fell out of favor. But introspection as a source of data has never been abandoned. Even Watson conceded that psychology needed to explain verbal reports: they are, after all, behavior (e.g., Watson, 1925, p. 6; see Boring, 1957, p. 645). Surely a self-report is no more than an introspection made public, and this century's social science has depended heavily on such data. Introspection (the method) did not disappear--it has only been revised and renamed. The historical objections, suitably adapted to current procedures, still carry weight, and reappear in the recently revitalized assault on self-reports as a source of psychological data.

In 1977, Nisbett and Wilson published a paper in the Psychological Review, in which they claimed that subjects have "little or no direct introspective access to higher order cognitive processes" (p. 231). This does not mean that respondents

will not answer a researcher's questions, however. People queried about, say, how they add three-digit numbers will describe a plausible way such figures can be processed correctly. Or if that example is unpersuasive, consider questions such as, "How do you recognize a friend?" "Why do you like your roommate?" "What kinds of commercials are most effective?" Nearly any undergraduate can give answers to these questions, but such data have very limited value. Nisbett and Wilson claim that when subjects are put in a position where they have to "tell more than they can know," they construct plausible answers which are based not on their specific cognitive experience, but on their theories of arithmetic, perception, liking, persuasion, and so forth. And if the subject has no access to what or how s/he really was thinking during the behavior in question, the respondent may very well believe in the veracity of the self-report. The contamination of reports by external theories and the fragility of memory, of course, were also problems (fatal ones, at that) for instrospective psychology.

Nisbett and Wilson (1977a) base their case on a number of studies (for fuller accounts of the empirical evidence than are in the original paper, see Nisbett & Wilson, 1977b; Wilson & Nisbett, 1978; Nisbett & Bellows, 1977; a later paper is Wilson, Hull & Johnson, 1981). The basic result was the same in each investigation: subjects could not say accurately why they made certain judgments. Sometimes people claimed to be influenced by factors which objective evidence showed were without effect. Sometimes people denied being affected by variables which statistical analysis revealed as important. Both types of failure are evidence that people cannot plumb their own depths.

For instance, Nisbett and Bellows (1977) gave subjects a sketch of a target person, and asked them to rate her on liking, flexibility, sympathy and intelligence scales. The information in the sketch differed from subject to subject. In addition to the ratings just mentioned, subjects also said how important certain facts (the varied information) were in their ratings. Another group--

the "observer subjects"--read a precis of the study and then estimated the importance of the varied facts for the actual subjects. The latter were very inaccurate in assessing the impact of the information on their liking, flexibility and sympathy ratings (comparison of different subjects' ratings, of course, gave an objective measure of the information's real effect). This is the first bit of evidence that people lack access to their higher cognitive processes. The second bit is that the observer and actual subjects had very similar (r was about .80) estimates of the information's impact. Observer subjects obviously had no cognitive history to report, for they did not make the other ratings; nonetheless, they generated the same data as the subjects who were supposed to have privileged access to their cognitions. The third piece of evidence is that subjects were accurate in predicting the results of the varied facts on the intelligence ratings--but the observer subjects were equally precise. Nisbett and Bellows say that people have better-developed naive theories of intelligence than of liking, flexibility and sympathy (or perhaps the intelligence information was just more obvious). Therefore, everyone "knows" how people made the intelligence estimates. Thus, the Nisbett and Bellows study supports the notion that "privileged access" subjects are no better than anyone else in describing their judgment processes, and that this is true both when everyone is inaccurate (sympathy, liking, flexibility) and when everyone is accurate (intelligence).

This experiment is fairly representative of the others in Nisbett's program. Several features of these studies have been criticized, and the objections will be taken up in the following section of this paper. For now, readers should particularly note these things: (1) that a between-subjects design was used, so that comparisons are between groups of people, not within people; (2) that the study was designed to fool its subjects, by using implausibly important information which previous research had shown to be effective in altering the ratings; and (3) that respondents did not estimate the impact of the information at exactly

the same time they made their ratings, since a few minutes elapsed before the second set of responses was requested.

These studies are not the only evidence against the validity of self-reports. Nisbett and Wilson (1977a) also review literature on dissonance and self-attribution, on subliminal perception, on creative problem-solving and on helping behavior. This research leads them to several conclusions: people are often unable to report verbally about their motives and feelings in such a way as to explain their behavior (e.g., the student in a forced compliance study does not realize that s/he was subtly coerced into an arduous task); people cannot recall their original opinions after they have been persuaded; subjects often resist explanations given them in debriefings, protesting "that's not what I thought" when objective evidence suggests the opposite; people are predictably influenced by subliminal stimuli which they do not report having noticed; people forget all the hints and preliminary thinking that precede a "flash of insight;" and that people refuse to say that their willingness to help another person is influenced by the number of other potential helpers present. This is an incoherent and obviously carefully selected body of evidence, but if it alone would not prove Nisbett and Wilson's case, it certainly justifies the hypothesis that their research program tested.

We should be careful in noticing exactly what Nisbett and his colleagues claim. They do not say that people are usually (or even often) wrong in explaining the causes of their own behavior. Whenever their general theory of the behavior is correct, people will easily supply accurate accounts (recall the intelligence results in Nisbett & Bellows, 1977). But so long as both actors and observers give the same reasons for behaviors, the importance of privileged access to cognition is illusory. The problem is to find evidence that actors are right about the causes of behavior when observers are wrong (Nisbett & Ross, 1980, p. 211). Certainly actors will often have information observers do not, but the data may be unavailable to consciousness or may be sabotaged by poor inferences

(Nisbett & Ross, 1980, pp. 223-225). Worse, the behaviors may not have been performed consciously in the first place (cf. Rich, 1979), and the person may only think that s/he knew what s/he was doing. In any of these circumstances, actors asked to account for actions will be obliged to resort to fiction, which may or may not happen to be true. "We propose that when people are asked to report how a particular stimulus influenced a particular response, they do so not by consulting a memory of the mediating process, but by applying or generating causal theories about the effects of that type of stimulus on that type of response" (Nisbett & Wilson, 1977a, p. 248). Nisbett and his associates maintain that scripts or naive theories or expectations--not memory--control people's understanding of their own behavior. This view is very plausible.

The Defense of Self-Reports

But it has not gone unanswered. A number of responses to Nisbett and Wilson (1977a) have appeared (Smith & Miller, 1978; Rich, 1979; White, 1980; Cotton, 1980; Sabini & Silver, 1981; Adair & Spinner, 1981; Kraut & Lewis, 1982), and have caused Nisbett to make several concessions (see Nisbett & Ross, 1980, ch. 9). This section examines the defense of self-reports.

One criticism whose validity Nisbett and Ross (1980, pp. 214-216) seem partly prepared to admit is that several (though not all) of Nisbett's studies used between-subjects designs (Smith & Miller, 1978; White, 1980; Kraut & Lewis, 1982). For instance, suppose that respondents explosed to a manipulation do not feel that it influenced their judgments; comparison of those judgments with others made by subjects exposed to a different treatment, however, shows that the manipulation did in fact alter the first group's responses. Thus, no true awareness. This is an unfair analysis, say the critics.

The issue is individual, not group, consciousness. And in fact, Smith and Miller (1978) managed to calculate within-subjects correlations between actual ratings and estimated relevance from some of Nisbett's data. Their

reanalysis suggests that individual subjects may have been aware even if whole groups were not. But the correlations may only reflect rationalizations of high ratings (Nisbett & Ross, 1980, p. 215), and so are not conclusive: a subject noticing that s/he had made a high rating might just raise his/her estimate of all the informations' relevancies. But the criticism is still generally on target (cf. Nisbett & Ross, 1980, pp. 214-216), though it does not suffice to explain away all of Nisbett's results. Smith and Miller's correlations only explain about 6% of the rating variance. Only if this 6% were also the variance accounted for by the manipulations would this criticism be fatal, and we have little information on this point. One pertinent study (Kraut & Lewis, 1982) reports a partial r of .15 between actual impact and estimated relevance of person perception cues when controlling for the effects of lay theories. This is only 2% of the judgment variance, however; while it shows some evidence of awareness, it is hardly an impressive brief. At the very least, Nisbett's data show that any awareness effects in those studies were too weak to appear in group data.

A second criticism is that the manipulations are too devious. They are so subtle that no one could reasonably be expected to be aware of their effects (Smith & Miller, 1978). For instance, in one experiment (Wilson & Nisbett, 1978, exp. 2), people judged which of four (identical) pairs of nylon stockings was preferable. Though respondents denied it, the position of the stocking in the display was the key to the ratings (the sample on the right-hand end of the row was most preferred). Subjects gave knit, weave, sheerness and so forth as the reasons for their ratings. Critics charge that this manipulation (position) is unfair, and that the experimental results depend upon an effect which the whole design was intended to hide. Similar objections can be made to the manipulations in Nisbett and Bellows (1977) and other studies in the program. Kraut and Lewis (1982) observe tartly that the independent variables in Nisbett and Bellows explain only 3-10% of the variance in ratings. With more obvious manipulations subjects

might be more aware. This is probably true, but again not very helpful in defending self-reports. If verbal accounts can only be trusted for the obvious, what good are they? This criticism amounts to a concession that people do not have access to subtle effects on cognition. Ordinarily, these are exactly the effects of most interest to researchers. Nisbett and Ross (1980, pp. 214-216) grant some credence to this objection, but it really does little damage to their most important claims.

Though these two criticisms are the most common in the responses to Nisbett and his colleagues, some others have also appeared. Smith and Miller (1978) find a fundamental circularity in the case against self-reports. Nisbett and Wilson (1977a) argue that correct verbal reports are due to coincidental use of an accurate intuitive theory (e.g., a theory of how one tells that someone is intelligent). If the manipulation is, in the intuitive theory, a representative stimulus for the behavioral outcome, then the subject will be able to say that the stimulus caused the behavior. But the only way we know that a stimulus is representative, Smith and Miller complain, is that people accurately identify its effect on their actions. This is circular, and constitutes a serious weakness in the overall theory. The ideas of representative stimulus and plausible intuitive theory spiral in on themselves and do not provide a scientific account of accurate and inaccurate self-reports. But, it should be noted, this indicts the theory while leaving the central finding unaffected--it is just not well explained.

White (1980) has noticed another interesting logical problem. Verbal reports are behavior. Nisbett and Wilson (1977a) claim that people produce behavior without awareness. But Nisbett and his associates trust the self-reports to be accurate indices of subjects' (mistaken) consciousnesses. How can the case against self-reports depend on the accuracy of the verbal reports of reasons for behavior? Either people do have access to consciousness (in which case we may accept the self-reports and look elsewhere to explain the report/behavior discrepancies) or they do not (in which case none of Nisbett's self-report evidence

can be admitted). This contrast is not well drawn, however. Nisbett and his colleagues do not seriously maintain that people have no access (at all) to consciousness, and they admit that people do possess idiosyncratic information about themselves. Self-reports are not Monte Carlo outcomes--they are just systematically misleading under some circumstances. Nisbett admits that people have access to various cognitive contents, but says that this does not amount to a grasp of the whole cognitive process. A subject may know full well what s/he is thinking ("she is sympathetic") without knowing why the thought appears. The second awareness is more subtle than the first, and is the one mistrusted by Nisbett. The first awareness provides the self-report data. White's objection therefore seems negligible.

A criticism which must be taken seriously is that subjects may be perfectly aware of the causes of behavior during the action, but their later veral recounting does not capture that knowlege. Possibly the awareness does not remain in memory long enough to be reported even a few minutes after the behavior (Ericsson & Simon, 1980; Kraut & Lewis, 1982). Possibly the awareness has been stored, but is unavailable to recall for one reason or another (see Nisbett & Ross, 1980, ch. 2). Possibly subjects have access to the true reasons for their behavior, but simply have difficulty communicating those reasons in a particular self-report format (cf. Sabini & Silver, 1981, p. 172). Possibly subjects know why they performed as they did, but are ashamed to admit the true causes or feel the experimenter is seeking a different result (White, 1980; Adair & Spinner, 1981; Nisbett & Wilson, 1977a, concede that several of their studies could be explained in these ways). None of these criticisms is easily dismissed, and they will be considered again in this paper's final section. But if we distinguish between refutations of Nisbett and Wilson on one hand, and constructive justifications for self-reports on the other, these last objections clearly fall into the first class.

In sum, the criticisms of Nisbett's work fail to do mortal damage. Some speculative objections cannot presently be dismissed, but these have very limited constructive value.

An Empirical Study

The study reported in this section was undertaken with every expectation that it would provide clear evidence of accurate access to higher cognitive processes. Accurate self-reports are common, but as we have seen, Nisbett says they are due to respondents' use of plausible theories of the behavior being explained. Ambitious though it seems, the key idea in the study to follow is that no common theory of the studied phenomenon is likely to exist, so that many subjects will be unable to make "plausible" responses.

The phenomenon is two-mindedness--possession of two conscious minds. Some people are able to have dialogues within themselves, with both dialogic participants seeming to have full and free intellectual capacities. Two-minded people sometimes resolve difficult problems with such exchanges. Other people, those who are single-minded, have no such homunculi. Not all people report having known about the possibility of the other way of thinking. Single-minded people might naturally view "internal dialogue" as a metaphor; two-minded people do not have such dialogues all the time, and so would find single-minded accounts perfectly natural. Although some persons may have access to an implicit theory about mindedness, some probably will not.

Whether or not people have previously thought about two-mindedness can presumably be determined by a straightforward verbal report. This study also used a scale designed to measure mindedness. Assuming the validity of subjects' scale responses (that is, that they have and use direct access to their conscious processes), then whether or not subjects also have an implicit theory of mindedness should not affect the scale responses. Support for this hypothesis would strongly suggest the validity of verbal reports about cognitive processes, even in the face of a relevant implicit theory; such evidence would seriously injure Nisbett's claim that self-reports about cognitive processes are controlled by plausible theories.

Subjects were 149 students. All but two were undergraduates, and about 80% were seniors or juniors. 54% were male and 46% female. About two-thirds of the respondents were departmental majors.

Students responded to an experimental booklet which had several parts. The first section contained twenty brief passages, eight of which were designed to reflect two-mindedness (for instance, "I had about decided to ignore my appointments in favor of an early return home, but suddenly my conscience said, 'No!'"). The other twelve passages were fillers. Subjects rated the clarity and figurativeness of each passage on five-point scales. This part of the booklet was intended to provide evidence for the validity of the two-mind scale: two-minded subjects were expected to rate the dual consciousness passages as clearer and more literal. This effort failed, since scores on the mindedness scale had no statistically significant relationship with either the clarity or the figurativeness scores.

The next eleven items were the mindedness scale. Examples of items are, "Do you ever have conversations with yourself?" and "Do you argue with yourself?" The internal reliability for the scale is .69 (Cronbach's alpha).

The final part of the booklet contained some demographic items, and among other things, asked whether subjects recalled reading, talking or thinking about the topic of two-mindedness. 60% of respondents said they had never read anything about it, 58% had never talked about it, and 36% said they had never thought about it. Evaluation apprehension might depress these figures somewhat, especially for the "thinking" question.

The hypothesis was evaluated with t-tests. Subjects who reported not reading, talking or thinking about two-mindedness were one group, and those who had reported doing all three were the other. The second group had a significantly higher score on the mindedness scale ($\bar{X}_1$=21.6, $\bar{X}_2$=25.2, t=3.63, p<.001). Separate comparisons were run with each of the three awareness items, allowing more subjects

to be involved in the tests. Students who had read about two-mindedness had significantly higher scores than those who had not ($\bar{X}_1$=22.5, $\bar{X}_2$=24.6, t=3.14, p<.01). Students who had talked about the topic also had higher scores ($\bar{X}_1$=22.4, $\bar{X}_2$=24.4, t=3.24, p<.01). Similarly, students who reported having thought previously about two-mindedness scored higher as well ($\bar{X}_1$=22.2, $\bar{X}_2$=24.2, t=2.51, p<.05). The hypothesis is obviously not supported.

Nisbett and his associates would certainly explain these results as follows: students who had theories about mindedness would therefore know dual consciousness to be a natural phenomenon and would be more likely to make two-minded responses. In support of this, they could point to some additional evidence from the study. In the last part of the questionnaire, subjects classed themselves as one- or two-minded (this self-report correlates at r=.36, p<.001, with the mindedness scale scores). Use of this direct self-report instead of the scales scores as the dependent variable in the t-tests replicates the first pattern of findings. Subjects reporting no previous contact with the topic were far more likely to class themselves as single-minded than respondents who read, talked and thought about it (t=4.00, p<.001). Differences in the same direction fall short of significance for talking (t=1.40, p=.17) and for reading (t=1.71, p=.09), but acheive significance for thinking or not (t=4.48, p<.001). The more contact with theory students had, the more plausible they knew two-mindedness to be, and the more likely they would be to identify themselves as two-minded and to act in a two-minded fashion while reponding to the mindedness scale.

Unfortunately, there is another explanation--one which was unanticipated and therefore not controlled for. Possibly two-minded people think they are wierd or brilliant, or for some other reason seek out information about mindedness. (Single-mindedness is slightly more common, based on the self-classification: 52% to 48%). Although a comparison of single- and two-minded people on their responses to "What percentage of your fellow students do you estimate think like you?" actually

showed the two-minded respondents as being nonsignificantly more likely to estimate a higher percentage (t = 1.26, p = .10), this lack of pessimism could be a result of their contact with information about mindedness. It is still possible that two-mindedness causes awareness of theory, rather than exposure to theory causing reports of two-mindedness, as Nisbett and his colleagues would surely prefer.

In either event, this study has certainly failed to damage the case against self-reports, and may even have supported it. The two-mindedness construct is still being refined conceptually and operationally, and the failure to validate the scale by means of the passages-rating task is troubling. But in spite of the tentative nature of this report, we should probably not expect any assault on Nisbett's position from this corner.

The Worth of Self-Reports

The difficulty in evaluating the validity of self-reports which presume access to higher cognitive functions is that the evidence on both sides of the issue is equivocal. Many of the studies Nisbett and Wilson (1977a) review are susceptible to alternate interpretations. Demand characteristics or evaluation apprehension, for instance, could account for the reported results (Adair & Spinner, 1981). Findings of accurate self-reports (e.g., Wyer, 1973) can be explained without resorting to the assumption of accurate introspection. The criticisms of Nisbett's position identify some weaknesses in his case, but do not themselves establish its converse. Data analysis and experiments other than those conducted by Nisbett and his associates (Smith & Miller, 1978; White, 1980; Kraut & Lewis, 1982; the study reported in the previous section) also leave the matter unsettled. This final section therefore attempts to sort out what findings we may rely upon, join these with well-grounded theory about human cognition, and specify the conditions under which self-reports about cognition are least suspect.

First of all, we ought to notice that people do sometimes judge plausibility even when instructed to remember. Reder (1982) documented this unsurprising finding in an especially useful way. She asked subjects to read ten stories, and then to say whether test sentences had been in them or not (this requires direct retrieval) or whether the sentences were true or not (this requires a plausibility check). These judgments were made immediately after each story, after all ten, or after two days. She found that retrieval was quickest in the immediate condition, but that plausibility judgments were fastest after the two day delay. Using a complex mathematical theory, she was able to calculate the probabilities of using plausibility strategies in spite of retrieval instructions. The probability of this error (which, of course, is precisely the alleged source of invalidity in Nisbett and Wilson's [1977a] theory) is .21 in the immediate condition, .51 after all ten stories, and .73 after two days. We ought not place too much faith in these exact figures, since her own replication gave corresponding values of .00, .26 and .25; nonetheless, people obviously do often judge plausibility of an item--in spite of instructions--when it is easier to make the plausibility judgment than to seek specific memories. Reder's explanation of her findings is that when a memory trace is fresh, retrieval is faster and easier than plausibility strategies. But once the traces begin to fade, the advantage shifts to plausibility because a larger number of items in memory can begin and sustain the cognitive process. Retrieval judgments ("did I read this?") are always simpler and faster than plausibility judgments ("is this true?"), but plausibility's greater efficiency during the memory search stage of the task outweighs the judging advantage unless the right memory traces remain activated, as in the immediate condition. So we should expect people to use reconstruction rather than the requested recall processes more and more often as the delay between stimulus and search increases. If we expect our subjects to found their self-reports on specific memories, we must interrogate promptly.

A second pertinent chunk of evidence shows that people often behave mindlessly (Langer, 1978; Roloff, 1980; Berger & Douglas, 1982). Repetitive behaviors are often overlearned, and people find that they can follow the appropriate script without paying conscious attention to what they are doing. Gronbeck (1982) believes that common mental processes are also automatic: "Perceptual, material, semantic, pragmatic and ethical inferences are undoubtedly used commonly without thinking. The inferred-from statements are perceived as immediately apparent because this class of inferences is the product of our acculturation, of education in our fellow beings' linguistic, mental and behavioral customs" (p. 90). The possibility of mindless activity, either overt or private, has serious implications for the validity of self-reports. The request for a verbal report may artificially raise self-consciousness about the behavior (Berger & Douglas, 1982, p. 57; cf. Ogden, 1926, p. 192). This may result in a different behavior if the request is made before the activity, or may result in an excessively consistent depiction of self if the questions are asked retrospectively. If people perform a task without conscious awareness, presumably no memory traces of the underlying cognitive processes exist, and "reports" will therefore have to tap the subjects' implicit theories of the behavior, regardless of task instructions or motivations (Kellogg, 1982). Others have also noticed the connection between Nisbett and Wilson's (1977a) claims and Langer's notion of mindlessness (Wilson, Hull & Johnson, 1981; Rich, 1979). The conclusion suggested by these writers is that accurate verbal reports are only possible when people act mindfully.

As stated, however, this claim is too sharp and must be softened. For one thing, it is entirely possible that a subject can give an accurate verbal report of mindless behavior if both the behavior and the report are controlled by the same script. The information in that (common?) case will be perfectly accurate, and any otherwise valid inference made from such data by the researcher

will remain valid, so long as the investigator does not import consciousness into the report or the behavior. A second problem with the mindlessness conclusion is posed by an interesting study which shows that people can give verbal reports based on things they cannot remember. Jacoby and Witherspoon (1982) demonstrated this using both normal subjects and memory-impaired (Korsakoff) patients. All subjects answered questions which contained a homophone used in a rare way (e.g., reed instead of read), and then spelled words which were also presented orally (these words included the homophones, of course). Finally, respondents were asked if they recognized words in the second task from the first one. Spelling, as one would expect, was biased in favor of the homophone versions used in the first task; this was true for both normals and memory-impaired people. The normals, however, reported recognizing the second task's words three times more often than the amnesiacs. So although the patients' memory for the words was very poor, their verbal performances reflected unimpaired cognitive processes. This study shows that inaccessible cognitive content can influence verbal behavior; obviously, no plausible theory of spelling or word association could help the amnesiacs. This implies that behavior, presumably including self-reports, may not be as strongly affected by memory traces (hence by consciousness) as the mindlessness literature suggests. A similar conclusion is warranted by the work on subliminal perception reviewed by Nisbett and Wilson (1977a). The idea of consciousness is intimately related to that of memory (viz., we can only be conscious of things which register in short term storage, though cues can be from the environment or from long term memory or from both), but we are evidently capable of acting on the basis of unstored or inaccessible information. These considerations weaken the claim of a simple association between mindlessness and invalid self-reports: one behavior need not be consciously heeded in order to affect a later behavior. Having made this stipulation, however, it remains to be said that the mindlessness literature is generally persuasive, and that we should expect valid reports to be more probable with consciousness than without it.

For repetitive scripted behaviors (i.e., mindless ones) reports will not be drawn from actual memory. The accuracy of self-reports in such cases will be determined by the degree to which the same script controls the original and verbal behaviors. The selection of that script during the verbal reporting could be influenced by various availability and representativeness heuristics which would not lead to veridical descriptions of the cognitions producing the behavior (see Nisbett & Ross, 1980, ch. 2).

The effects of consciousness and memory on retrospective verbal reports are crucially affected by the passage of time. Ericsson and Simon (1980), in a very strong theory-based literature review, insist that questions should be asked as soon as possible--perhaps even during the behavior. They spell out a number of conditions under which concurrent reporting should not affect the actions under study. Thought listing and think-aloud protocols have the advantage of immediacy, and will therefore consistently produce better data than retrospective reports (cf. Adair & Spinner, 1981). Ericsson and Simon's position is entirely consistent with Reder's (1982) results. They claim that with delayed probing, subjects will be tempted to report a generalized theory of what they did, rather than going to the trouble of searching for a specific memory. Investigators should guard against memory loss or interference. Information can be flushed from short term memory by negative reinforcement, time or routinization. A question's wording may force an inference, or push the memory into an inauthentic form (cf. Adair & Spinner, 1981; Nisbett & Wilson, 1977a). To obtain a fully valid self-report, therefore, the investigator must ensure that the information is in memory and that the experimental conditions facilitate unimpeded retrieval. Behaviors performed without consciousness, leading questions, delayed probing and intervening thought will all conspire to generate invalid verbal data. Ericsson and Simon (1980, pp. 246-247) indicate that all of Nisbett and Wilson's (1977a) empirical evidence has one or another of these flaws, and so Nisbett's

claim that people have little access to higher cognitive processes is unacceptably broad and must be qualified. Though the lists of circumstances under which invalid verbal reports are expectable is similar in both these papers (as is that in Adair & Spinner, 1981), Ericsson and Simon offer a very different estimation of the evidence: ". . . verbal reports, elicited with care and interpreted with full understanding of the circumstances under which they were obtained, are a valuable and thoroughly reliable source of information about cognitive processes" (p. 247).

Ericsson and Simon's (1980) optimism seems better grounded than Nisbett and Wilson's (1977a) rejection of self-reports. Ericsson and Simon have a more coherent account of valid and invalid verbal data. Their theory is built upon relatively uncontroversial findings regarding memory, and makes errors expectable under certain straightforward conditions. The Nisbett and Wilson explanation, on the other hand, does not always withstand careful examination. We have already reviewed Smith and Miller's (1978) discovery of the circularity regarding what is a plausible implicit theory. Another curiosity is Nisbett and Wilson's concession that people have privileged access to cognitive content. How else can a process be represented than as a succession of contents? (Smith & Miller, 1978; White, 1980; also see Sabini & Silver, 1981). To admit that people can report or call up content while denying that they can do the same for cognitive processes requires a far more sophisticated theory than Nisbett and Wilson provide. Though the process/content distinction may ultimately prove to be tenable, in its present form it is not. Ericsson and Simon's theory avoids the logical problems of Nisbett and Wilson's explanation. At the same time, Ericsson and Simon can account for all of Nisbett's empirical results. Perhaps most persuasively, nearly all the criticisms and modifications of Nisbett's views--reporting delays, demand characteristics, mindlessness, plausibility searches and so forth--occupy central, not appended, positions in Ericsson and Simon's theory. Experimental

evidence does not unequivocally support one theory over the other, but the superior coherence of Ericsson and Simon's position makes it preferable. By considering only the time of verbalization relative to the task, and the correspondence between the requested information and the information normally heeded during the task, they build a strong case that inaccurate verbal reports are avoidable in carefully designed research.

Conclusion

The case against self-reports seems therefore to be really a case against some self-reports. Certainly some verbal responses are not valid. These tend to be retrospective reports and/or those given in response to inappropriate questions; similar problems were observed earlier in our discussion of introspection. Nisbett and Wilson's (1977a) explanation of invalid recountings relies on a questionable distinction between cognitive process and cognitive content, as well as tautological notions of plausibility and representativeness. Ericsson and Simon (1980), in contrast, give a systematic and internally coherent account of the validity of verbal reports. It needs to be emphasized, however, that the choice made here in favor of Ericsson and Simon's theory is not data-based. Though Nisbett and Wilson's studies are flawed in various respects, no clear counter-evidence has appeared. Smith and Miller (1978) and Kraut and Lewis (1982) present data which tend to weaken the Nisbett position, but these papers are not beyond criticism themselves. The study presented here did not yield any sure claims either, but if self-reports and implicit theories were completely unrelated, that study had an excellent opportunity to demonstrate that proposition. So empirical research does not help us very much.

The worth of self-reports depends almost entirely upon the procedures used to elicit them. Argumentation scholars, like all other cognitive researchers, ought to be wary of certain potential problems: letting even a few minutes elapse

between a thought process and a report of it; seeking data which were never in consciousness; allowing a plausibility search to be a tempting alternative to retrieval efforts; asking for a general description of process when subjects only experience a specific episode; and allowing evaluation apprehension or demand characterisitics to influence reponses. Data collected in violation of these suggestions is not useless, however; it simply is unlikely to reflect privileged access to cognitive processes. It may still be extremely useful in describing implicit theories about argumentation. These essentially public constructions are at the core of some recent and important research (Jacobs & Jackson, in press; Goodwin & Wenzel, 1979). Gronbeck (1982) has even argued that logic itself ought to be seen as a product of socialization. Other current efforts to describe interpersonal argumentation may also profit by use of verbal reports which do not access specific cognitive experience. We need only be cautious in what we claim about the generation of such data.

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